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MAGNETIC AND INERTIAL CTR: PRESENT STATUS AND OUTLOOK

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Introduction

As the last after-dinner speaker this evening, I am obviously placed between satiety and somnolence, weighted toward the latter. Keeping this in mind, I'm going to mix a large portion of political material with a s ·ll amount of technical matter in this talk, since it is well-known that p. litics can elicit semblances of mental activity long after the higher centers of the brain have retired.

I'm going to be speaking of the present status and outlook for both magnetic and inertial CTR, relative to each other, as well as by themselves. I will be concerned with not only the outlook for the next couple of years, which for the particular case of laser fusion was just most ably covered by my colleague John Nuckolls, but with what we in the fusion community must do now to assure that the longer term future will unfold properly. In all of what follows, I wish to have it very clearly understood that I will be expressing strictly my own opinions, which are not necessarily that of the AEC, my Laboratory or anyone else—they may even be publicly denounced by me, a day or two after I return to Livermore.

The Current Basis for Optimism in Fusion

I'm going to start off with some good news and some bad news, as is the custom these days. The first viewgraph presents the basic good news about the current status of the various areas of controlled fusion research. All across the gamut of approaches to CTR,[1] from magnetic mirror machines to laser implosions, we're just about there—the Promised Land of the Lawson "scientific breakeven" criterion is definitely in sight. Indeed, for those of

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us in the inertial CTR side of the family, we've already made it this past year, in the persons of our front-running Soviet colleagues. The Basov-Krokhin-Sklizkov group at the Lebedev Institute have imploded a sphere of 100 micron initial radius of thermonuclear material deuterated polyethylene-to a density of about 30 grams/cm^3, with a corresponding Lawson number of about 3×10^{14} sec/cm^3, and produced a DT-equivalent peak thermonuclear power in excess of 10 megawatts, a new record for most CTR devices.[2] In a related development, workers at KMS Fusion won a place of honor in the CTR community during 1974 by carrying out the first reasonably unequivocal production of thermonuclear energy via laser-energized implosion of fuel by any effort anywhere,[3] and in the process established new U.S. CTR community records for both Lawson number and peak thermonuclear power production. Both of these were indeed most notable achievements.

Meanwhile, there was a stream of unusually glad tidings from our colleagues in the magnetic confinement end of the business. Not only were the present generation experimental fusion systems performing ever better—with higher densities, temperatures and confinements times, but the creation of the technology for the next generation of experiments has been proceeding without delays, thanks largely to recent budget increases. In a matter very important for the longer term, theoretical tools in both magnetic and inertial communities have been brought into ever closer, but unstrained, conformity with key experimental results, providing the basis for increasingly confident predictions of scientific breakeven experiments in this decade, and perhaps as early as 1977. Indeed, inspection of the evolutionary tracks of all major CTR approaches shows that substantial closing of the gaps between presently realized $n\tau$ values and the "scientific breakeven" Lawson value of about 10^{14} sec/cm^3 is programmed to occur this year.[1]

A Guarantee of Success

The final portion of the good news is somewhat, but not completely, exclusively for those of us working in inertial CTR, and it is simply that ultimate success through scientific, engineering, and probably economic breakeven is apparently absolutely guaranteed us in our quest for scientific breakeven, by scaling the now-ancient success of Teller and his collaborators in burning inertially confined fusion fuel[4] into the DT microexplosion regime. Everyone knows that Teller & Co. burned deuterium via inertial confinement at a non-negligible efficiency, from published information on the size and yield of thermonuclear explosives,[5] and incidentally that is all that is indicated on the Figure.[1,5] In addition, we have Teller's assurance given at the IQEC in Montreal two years back and reaffirmed in his talk this morning, that the laws of inertial thermonuclear physics scale under the product of the fuel density and the fuel scale length, just as we would expect from basic principles. We therefore have ironclad assurance of ultimate success in the pursuit of scientific breakeven results for inertial CTR, simply by following Teller's own prescriptions and by scaling from macroexplosions to microexplosions, as he has indicated.

Until very recently, though, we were very pessimistic that this Telleresque approach could possible be carried from the scientific breakeven point to the economic breakeven one, mostly because of extremely stringent considerations of fuel pellet cost—a pellet producing 10^7 joules of fusion microexplosion energy can cost only about 0.1¢. This past year, however, that magician of matter manipulation, Chuck Hendricks, appeared on the laser CTR scene, and gave convincing assurance that he could crank out the required assemblanges of matter for us at prices that inertial CTR applications—both the well-known and the Telleresque approaches—could afford. This was a major breakthough. Also during this past year, Jack Daugherty, the gentleman in charge of large laser operations at the Avco Everett Research Laboratory, gave convincing assurances that he could deliver the laser systems of the efficiency, firing rate, beam quality, pulse duration, etc. and cost required to make this Telleresque approach a viable one for inertial CTR. While we have long cherished high hopes for the eventual utility in CTR of the awesome toys of Jack and his comrades, these assurances and what stood behind them likewise constituted a very large leap forward for the Telleresque approach, and for all inertial CTR. The resulting situation can best be summed up by the remark made by my colleague John Nuckolls to a closed inertial CTR meeting this past Fall: "If I were an energy program manager in Washington right now, I'd put every cent I had and could lay hands on behind this approach to CTR".

However, since there are still some low-level but legally potent bureaucrats in Washington who would prefer to have all public discussions of inertial CTR as far removed as possible from real world considerations, I'll terminate this brief discussion of why we in inertial CTR are looking so smug these days at this point.

Convergence of the Two Great CTR Systems

The good news for magnetic CTR from this subject area comes from the Avco Everett Research Laboratory contribution—the super-high power, high energy laser pulses. Preliminary calculations suggest that these pulses can be used to rapidly crush—after Bill Mead's talk this morning, perhaps the more apt word is crumple—moderate aspect ratio shells of solid DT, in the process shocking them up to mean ion temperatures in excess of 10 keV. This great shock heating may be made to occur even though the highest density seen by any of the shell is still comparatively low (of the order of 100 times solid density), and very little thermonuclear burn ensues as hydrodynamic disassembly very rapidly occurs. If, however, the exploding debris is then caught in a multi-Tesla magnetic field of appropriate confinement configuration, in a strongly scaled-up version of the neat experiments Alan Haught and his collaborators have been doing for some time,[6] the already ignited DT plasma will apparently burn to a usefully great extent as it slowly escapes the confining magnetic field. Initially, shock-heating the plasma well into the ignition regime has the important advantage over other laser solenoid fusion schemes that warm electrons don't have to heat cold ions into the ignition regime, a relatively slow process. An initially ignited fusion plasma need be magnetically confined only about 10% as long as a laser solenoid fusion plasmas of comparable density in order to produce the same fusion energy. This appears to me to be one of the points where laser and inertial CTR schemes are already meeting—a laser-driven implosion shock-heats a dense DT plasma to ignition temperatures, which then expands and burns efficiently efficiently in already-demonstrated magnetic confinement systems.

The salient problem I currently perceive with such an approach is the same nasty one that afflicts all magnetic CTR schemes to varying extents. It occurred to me when I was trying very hard to make large CW gas dynamic lasers (GDL) do something useful for CTR, back many years ago (when all the high power laser business was terribly hush-hush). It's not exactly a Wood's Terrible Theorem type of item, but rather something of a Lowell's Cruel Corollary, and it has a strongly economic flavor: Stripped of its bells and whistles, it says that if you compute the ratio β of the plasma pressure to the magnetic pressure in a magnetic CTR system, and time-average this quantity over the entire magnetized volume of the confinement systems, interior to the magnet coils, the resulting space- and time-averaged quantity $\langle\bar{\beta}\rangle$ had better be $\lesssim 10^{-4}$, or you can't afford to build the device, relative to the electricity you get from operating it, even when the price of superconductor and supporting structure is assumed to be 10 times cheaper than presently, which may be realistic for 1995.

I couldn't quite compose any GDL-heated, magnetically confined type CTR system that could survive this criterion. I rather hopefully shared this little item with John Dawson and Ray Kidder when, with the information on pulsed high energy laser systems that Ray's Eighth Card access then provided, they proposed what is presently called laser solenoid fusion. However, they can't evade it either, getting ground between ... and tolerable first wall neutronic loadings, as I had. They simply had too small a plasma volume, relative to their huge volume of magnetized neutron shield inside the superconductor. The scheme I just suggested gets around the Corollary crunch by using Daugherty's really <u>big</u> laser pulses to create an intermediate density plasma volume, which is of the order of 1% of the total magnetized volume of the entire system, <u>and</u> involves high levels of laser firing rate, which Jack's lasers can handle. This scheme just involves decreasing our plasma surface-to-volume ratio to levels we can economically live with--if everything else can be made to work, which a first inspection suggests may be in the cards.

Limitations to CTR Confidence

The bad news for CTR is simply a quite uniform observation forced by the good news, and is indicated in Figure 2. As the child who noted that the emperor indeed had no clothes might (when he got a little older), "If you're at the Lawson breakeven criterion, or even within a few orders of magnitude of it, and you're indeed using thermonuclear plasmas in your experiments, why isn't thermonuclear power pouring out of your fusion machines?".

The answer, simply stated, is that many of us have worshipped the magic Lawson criterion for so long (and incidentally taught everyone else to do so) that we have tended to forget that our lovingly confined plasmas need to be rather hot, in addition to everything else. In Figure 2, the cross-hatched circles pertain to experimental results already in hand, while the open circles represent confidently projected results of the next 1-3 years. The contours are of course lines of constant $n\tau$ [illegible], which is just the fractional fuel burn-up occurring during the burn event—Lawson breakeven corresponds to about 1% fuel burn-up, as is well known.

It is immediately apparent that those experiments that have the needed ion temperatures--5 to 10 keV--don't have the needed $n\tau$ products, being substantially lacking in the burn rate that only relatively high ion temperatures can confer. Of course, the response of the orthodox is something along the lines of "Ion temperature is easy to get; it's high $n\tau$ values that are the real figure-of-merit of a CTR approach". An agnostic might query in response, "If temperature's really so easy and $n\tau$ so hard, why didn't you have high temperatures before high $n\tau$, or, at the very least, at the same time?" Indeed, some people such as my colleague Harry Sahlin, a plasma focus devotee, are wont to remark that the $n\tau$ value of a well-constructed Dewar flask of liquid deuterium is of the order of 10^{30} sec/cm^3, 16 orders of magnitude above the Lawson criterion. I will let the matter go with the reminder that the recent Lebedev laser implosion results were well above the Lawson criterion, but nobody's making pilgrimages to Moscow because of them.

The other facet of the bad news is that almost all of us are wretchedly inefficient in getting electrical energy into hot, high $n\tau$ plasma. It's a good day when most of us get 0.1% of our power supply energy into internal energy of our thermonuclear plasmas, and a more typical day sees all but about one part in 10^4 being wasted in laser media, solenoid coils or whatever. It's partially for reasons like this that Jerry Yonas, whose relativistic electron beam machines are at least an order of magnitude more efficient than our lasers hopes to inherit the Earth of CTR; thus far, however, he's just getting the dirt.

What responses do we have to this bad news, other than a usually quite effective collective determination to steadfastly ignore it? With respect to low ion temperatures, I for one am willing to stipulate that it is indeed a real problem, even for we inertial CTR types, who have had any experimental ion temperature at all for less than a year. In the same breath, however, I assert my firm belief that it will soon be a problem of the past, for all CTR approaches.

John Nuckolls just pointed out to you that we in the inertial CTR area have been conducting experiments with an incredibly small amount of laser energy effectively incident on our fusion targets at most a few dozen joules, at peak effective laser powers of the order 0.1 terawatt (i.e., 10^{11} watts). These powers are three orders of magnitude down the peak laser powers we expect to be required for scientific breakeven experiments, and more than one order of magnitude below where we originally thought it made much sense to commence any laser fusion experiments. The energies deposited in the target, as John noted, are of the order of 0.1 joule/nanogram, and, from simple heat capacity arguments, are expected to bring the target to only one kilovolt temperatures, far below where significant thermonuclear self-heating, or bootstrapping, can be expected to help in the DT plasma energy budget. However, laser energies and powers higher by only one order of magnitude—which we confidently expect to have available for experiments this year from laser systems even now in the final check-out phase—will result in several kilovolt temperatures, when hydrodynamic losses are taken into account; correspondingly, the two order of magnitude greater peak laser powers we anticipate having in 1976 will result in mean deposition energies in the target of 1 joule/nanogram well into the DT ignition regime at reasonably high nτ values, solidly paving the way to scientific breakeven work in 1977. I speak rather glibly of bounding up 1, 2, and 3 orders of magnitude in laser peak powers-I assure you all realize from John Emmett's talk this morning of the enormous efforts involved in doing so, when working in the terawatt regime, i.e. at laser power outputs comparable to the entire electrical power generating capacity of this planet.

The plasma heating efficiency problem of inertial CTR is not so readily dismissed. Most of us in laser fusion are working with Nd:glass systems whose operating efficiency is two orders of magnitude below that which we've always considered minimal for CTR power plants. Vastly more efficient and higher firing rate-lasers will assuredly be required for power plants, and we have yet to demonstrate the operation of a laser having all the requisite qualities. Of course, as I mentioned before, all aspects of the laser problem are seemingly well in hand for the straightforward, low technical risk, Telleresque approach to inertial CTR-which is paradoxically the one we are precluded from openly discussing. We are also making real progress in obtaining the type of laser needed for the well-known and perhaps more elegant approach to laser inertial CTR, as John Emmett related this morning.

The resolution of the plasma temperature and heating efficiency problems seem equally straightforward for magnetic CTR. During the next 24 months, neutron beam injectors of unprecedentedly high power will be brought to bear on heating plasmas for quasi-CW confinement systems, such as the Princeton Tokamaks and the Livermore mirror devices. There is every reason to expect these means to be ultimately sufficient to raise plasma ion temperatures to the several keV level necessary for genuine ignition of DT and subsequent thermal bootstrapping. In the nearer term, these efficient "thermal support stockings" for magnetically confined plasma will apparently be sufficient to effectively reach scientific breakeven conditions via the "two-ion temperature" or "wet-wood-burner" mode. The plasma temperature problem is thus seen to be one which just needed large technological resources brought to bear on it. This has been done—and as a practical budgetary matter, could not have been done until recently—and the corresponding solution is apparently now well in hand.

For quasi-CW devices, at least, the problem of the relatively enormous quantities of energy used in generating the required magnetic fields will of course be obviated by the use of superconductors; indeed, magnetic CTR machines presently on the drawing boards have provisions for inclusion of such field-generating means. The analogous solution for pulsed devices, such as θ-pinches, plasma focus systems, etc. presently seems somewhat less clear, though.

Net Assessment of the Current Technical Status of CTR

The upshot of the foregoing is, I suggest, that the good news is real and substantial, and the bad news is illusory or of a transitory nature, for essentially all of CTR. The sum total of the good news and the bad news is that the way to scientific breakeven and a good distance beyond seems clear of major obstacles for one or more major approaches in both magnetic and inertial CTR. Specifically, I challenge anyone to present a respectable technical argument against the probable attainment of scientific breakeven in the present decade. But what is this "imminent breakthrough" status worth?

The Non-Technical Status of CTR

For one thing, matters in the world of affairs are seldom decided by technical arguments, even when the matters at issue are highly technical in nature. In particular, the public funds we in the CTR community spend on our collective vocation are given us by political leaders who, with a few notable, most happy exceptions, have little or no idea how we are going to spend it, or to what ends, and frankly care very little about the whole matter. For instance, we look ahead for many years to a few decades, working for the advent of commerical fusion power, while they look ahead many months to a few years, preparing for the next time the voters decide whether they professionally live or die. Our goals, our world views, our very existences are nearly completely disjoint from theirs to the basic satisfaction of both groups of us. The coupling between our universes occur only annually when a few of us tell a few of them how much money we need to do what, on which time scales, and why. However, these political leaders also listen extensively to other types of people in deciding how much support and impetus fusion research gets. Some of these people speak with understanding, wisdom and vision. For peculiar reasons, however, most of them fall into one of the three categories listed in Figure 3: the negative thinkers, the short-sighted and/or small-minded, and the outright incompetent.

For the remainder of this discussion, I will be drawing parallels ~~of~~ degrees of validity which you may decide for yourselves-between the drive for military fusion power of a quarter century ago, and the one for civilian fusion power in which we're presently engaged. (Some of you may be so unkind as to note that I am barely over a quarter century in age myself, and think to yourselves that more than a little panache on my part is required to speak so confidently of events of the late 40's, which took place before I could even do arithmetic. However, panache is a personal commodity I've never been terribly short of; also, fell circumstance has cursed me with being a repository of recollections of a number of the participants in this last big push for fusion power, so that I'm considerably better informed that my age entitles me to be.)

Your probably all recognize the sources of the three opinions in the "Then" column; they are close paraphrasings, respectively, of a famous report of the General Advisory Committee to the Atomic Energy Commission, and of Congressional testimony of two distinguished Directors of the Los Alamos Scientific Laboratory (who these latter two gentlemen are I'll leave to you to guess, given the hint that LASL has only had three Directors in its very long and distinguished history, and that one of these three is most known for being very enthusiastically in favor of everything, most particularly increased funding). With respect to the "Now" column, I will very non-coyly tell you that the bottom remark summarized the report to the National Security Council of an ad hoc group of the White House Energy Policy Staff, that the top one is a consensus of the people who presently very successfully market non-fusion forms of energy, and, to my prolonged sorrow, that the middle one represents a recent point of view of the most distinguished of the former Directors of the Lawrence Livermore Laboratory.

I submit to you the proposition that the principal hurdles between mankind and the advent of commerical fusion power are no longer technical ones, but rather political ones created and maintained by a majority of those at the interface between the fusion program and the public treasury. This majority—whose attitudes then and now I suggest are characterized by these paraphrasings—were and are, for whatever mixture of motives, able to discern very clearly the outstanding virtues of chemical and fission forms of energy, but can develop an appreciation of the merits of fusion energy to at most a negligible extent. They were badly wrong then, and I submit that they are badly wrong now.

As scientists working for CTR, we must do the very best technical job that we can to move toward fusion power, and I sincerely believe that we are presently performing rather splendidly. This is not idle boasting—our recent record of technical achievements will prove it to the most skeptical. As citizens, though, we must be much more effectively concerned with vaulting or eliminating the non-technical hurdles to fusion power-with educating, converting, or removing the wrongly persuaded majority at the fusion science-politics interface, who have so effectively limited the scope and pace of our efforts for so long. Let me suggest to you what our future might look like, again by recalling the past of Teller and his colleagues, suggesting once more basic parallels between the two fusion power programs.

Comparative Historiography of Fusion Power

Figure 4 notes the early-program similarities between the military and civilian fusion efforts in the U.S. John Nuckolls and I have long believed that the CTR problem as it stands in the present epoch will be best and most rapidly solved by taking three key pages from Teller's book:

(1) use as means a keenly perceived National need for fusion power, neutralize the political opposition to it, and thereby realize at least a measure of carte blanche for the effort;

(2) with whatever degree of carte blanche is available, bring as overwhelming levels of resources as possible to bear on the technical problems, including having the best people in the largest numbers working all viable paths to solution, for all technical problems in the real world are inevitably much tougher to crack than perceived at the outset; and

(3) massively overdesign your experiments, leaving as little to chance as possible, and throw elegance to the winds, for things always go wrong if they can.

Implementing this three-component strategy, it seemed optimal to Teller then, and seems so now to us, to have an intermediate stage of experimental work in the near term to prove principles, check calculations and instruments, and, not quite incidentally, to establish benchmarks of both technical and political value—he and we designate such experiments as "GEORGE". Intended only as an educational step on an exponentiating path to the ultimate goal, we expect to be continuing a series of GEORGE-level experiments at the Livermore Laboratory in the coming year. With the knowledge and capability gained thereby, we expect a second dawning of the thermonuclear age with the first MIKE-level experiments in 1977—if our magnetic confinements friends don't get their first: again we employ Teller's nomenclature for the level of experiments that remove all doubt as to the likelihood of ultimate success in the fusion power program.

Technical successes were far from completely decoupled from political ones, or vice versa, in the quest for fusion power a quarter century ago, and there is no reason to expect them to be in the present epoch. We of the fusion community must prepare now to politically exploit our technical successes of the next few years. Otherwise, they will crumble into political ruins before we can use them to rapidly extend technical advances, as victory always does for those who don't prepare for and use it. What should we do?

A Properly Paced National CTR Program

A minimal step is to call for a strongly accelerated, high priority level National CTR program, along the lines indicated in Figure 5, in a coherent, effective fashion. These are pretty standard milestones, but the timings are strongly compressed, relative to present official schedules. I assert, however, on the basis of considerable study and thoughtful judgement, that they are realizable, if this Nation decides to pursue fusion power the way it did the Manhattan Project and the Apollo Program.

Note in this program two items leading off in the direction of fusion-fission hybrid systems, generating large specific quantities of both fissile fuel for light water reactors (LWRs) and tritium. Hybrids are, I suggest, more than a personal eccentricity; they will help pay the substantial economic and political bills which an accelerated fusion power program will inevitably accrue. Along with very valuable, early experience in fusion reactor engineering, they will, if extensively deployed, substantially lessen the perceived National need for an intermediate nuclear power system (e.g. fast breeders) between LWRs and pure fusion power, as they will close the "fissile material gap" presently estimated to open in the late 80's and 90's. Where hybrids fit in the fusion scheme-of-things is seen in Figure 6, which summarizes some recent work of my colleague Jim Maniscalco.[7] Note that hybrids become <u>very</u> useful beasts at, and even a bit before, the CTR scientific breakeven level, even when it is assumed that they are to be electrically self-supporting.

Components of a Major Effort in CTR

Now consider, if you will, what some of the features of a really high priority National fusion power effect might be, as suggested in Figure 7. I suggest that such a program might be profitably modelled after recent successful major National technological efforts, such as those sponsored by NASA and the DoD. This Nation assuredly has the money, the people, and the know-how to make crash technological programs succeed, once the scientific basis is laid, as we are doing now and in the next few years in CTR.

CTR Program Costs: Comparative Economics

What's the thrust of all this? Figure 7 presents the answer graphically. It's that we need <u>more</u> money, an <u>awful</u> lot more money, <u>much</u> more than we're going to see in the foreseeable future, unless we ask for it reasonably, concertedly, persistently and, if we must, loudly. When we consider that the recent and present National funding levels of CTR efforts are at one to two orders of magnitude less than the Nation rather routinely funds substantial, important technological efforts-such as Apollo, Minuteman, Trident, and the B-1 fleet-we see that we just aren't taken at all seriously. Our efforts are considered inconsequential, our leaders taken as clowns and our representations as children's antics.

We presently mean so little to the National leadership-and presumably to our fellow citizens whom they represent-that our total funding comes to less than 1% of the current OPEC oil price gouging-we, who offer the Nation and all mankind cheap, clean, inexhaustible energy in concentrated, easy-to-use form! How can they possibly spend nearly ten times as much on a single modern missile-launching submarine than they do on a year of our efforts-we, who can get U.S. foreign policy out-of-pawn to the Arabs forever? How can they possibly spend as much to put three men in lunar orbit once as we need to get to fusion scientific breakeven, thereby achieving gains in National technical prestige that will endure solidly for decades? How can they possibly spend ten thousand times as much on one (peacetime) year's soldiers' salaries than recent major CTR experiments cost, when successes of our efforts will mean so much more to real National security?

The answer, my friends, is that we simply aren't credible. Our promises aren't believed. Worst yet, a lot of people-maybe a large majority of them-have not even heard of us, or only very vaguely so, and have no idea of how close we are to really history-making successes. Our job as citizens (and perhaps as scientists, as well) is no more or less than to educate our fellow citizens, as well as our common political leaders, in the basic facts of our work, its status and its promise, and to do so on a massive and continuing basis. Otherwise, we will necessarily continue as technological court jesters.

Moreover, if we don't warm up to these non-technical tasks and practice now, there's no way we'll be ready to capitalize on the victories of 1977-78.

The Basis for Confidence in the CTR Program

What do we do then, and how do we present our program? Figure 9a presents what seems to me to be the salient bases for our, and our fellow ctiizens', confidence in the CTR program. The fundamental points are that we know what we're doing and where we're going, as current experimental-theoretical accord testifies and theoretical predictions show, and that we're almost there, as our in-hand experimental results prove.

Figure 9b highlights some of the technical and personnel resources we presently have for attacking the last stages of the problem, if we can only muster the funds to do so with the necessary overwhelming force. Never before have been as close to our goals, or better equipped, or better staffed or better led. We just need wherewithal—the kind that has the signature of the Treasurer of the U.S. on it.

Figure 10 wraps things up on a philosophical note, with a license from Edward Teller to go to Congress for funding of CTR at a more reasonable level. We are surely "capable monomaniacs"; for no one would have got so far in such a desperately difficult field as fusion power if they weren't "capable"; we are obviously "monomaniacs", because no one would have worked in an area as long as we have as a community, in the face of such a small average rate of progress, if we weren't monomanical. Ergo, we have Teller's standing blessing to go in and ask for backing for a major technological effort in CTR. The last little aphorism of Arthur Clarke's is of course intuitively judged to be correct by most everyone associated for any period of time with technical endeavors and with the National Academy of Sciences, i.e. Congressmen: it may even be true. It's ours to use if Teller should attempt to revoke his blessing in mid-course.

Thank you.

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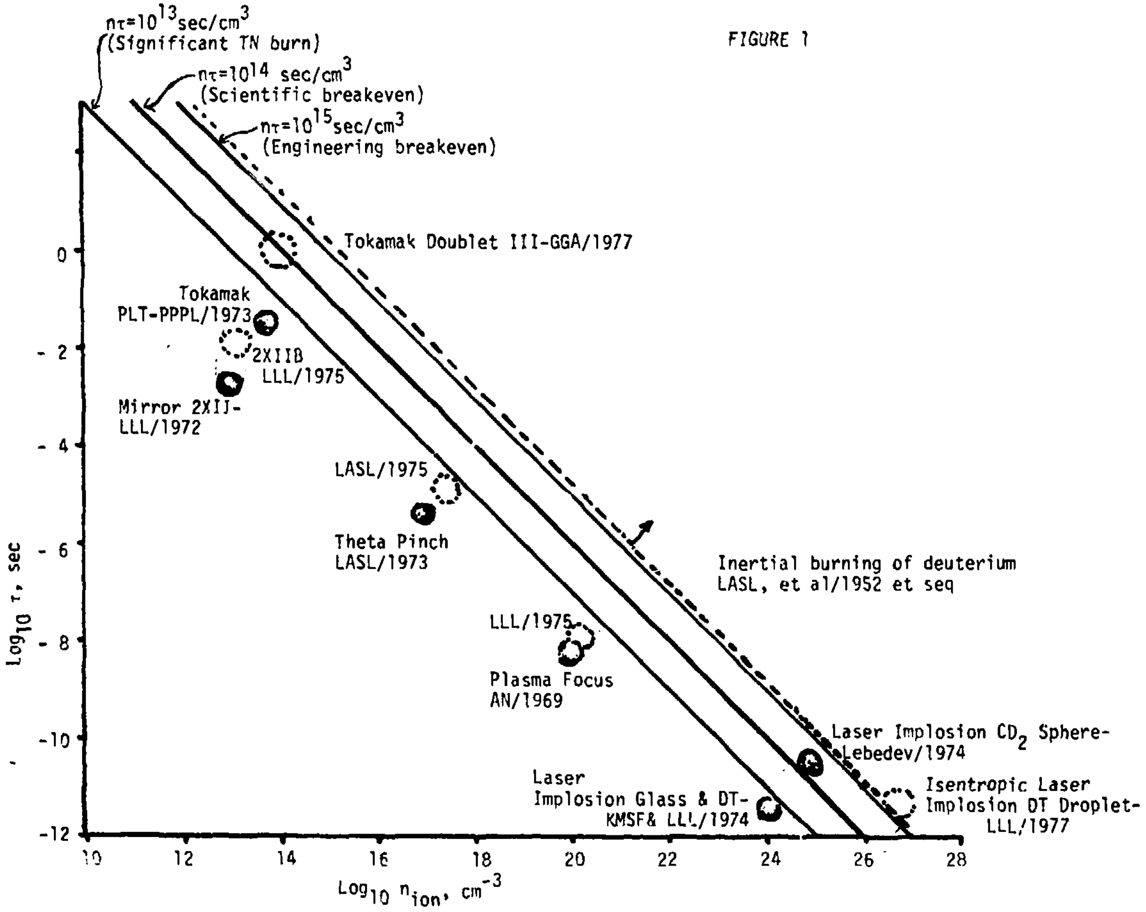
FIGURE 1



nτ=10^13 sec/cm^3 (Significant TN burn)
nτ=10^14 sec/cm^3 (Scientific breakeven)
nτ=10^15 sec/cm^3 (Engineering breakeven)
Tokamak Doublet III-GGA/1977
Tokamak PLT-PPPL/1973
2XIIB LLL/1975
Mirror 2XII-LLL/1972
LASL/1975
Theta Pinch LASL/1973
Inertial burning of deuterium LASL, et al/1952 et seq
LLL/1975
Plasma Focus AN/1969
Laser Implosion CD2 Sphere-Lebedev/1974
Isentropic Laser Implosion DT Droplet-LLL/1977
Laser Implosion Glass & DT-KMSF& LLL/1974
0
-2
-4
-6
-8
-10
-12
10
12
14
16
18
20
22
24
26
28
Log10 τ, sec
Log10 n ion, cm^-3

FIGURE 2

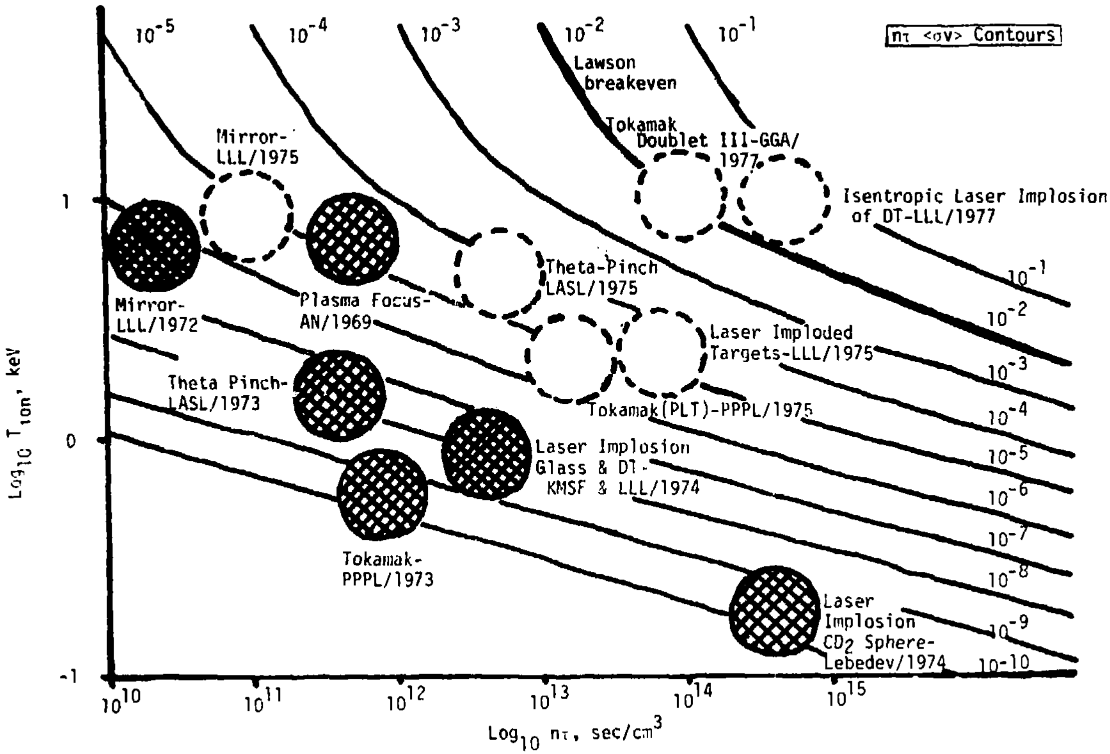

$n\tau$ <σv> Contours
10^{-5}
10^{-4}
10^{-3}
10^{-2}
10^{-1}
Lawson breakeven
Tokamak Doublet III-GGA/1977
Isentropic Laser Implosion of DT-LLL/1977
Mirror-LLL/1975
Mirror-LLL/1972
Plasma Focus-AN/1969
Theta-Pinch LASL/1975
Laser Imploded Targets-LLL/1975
Theta Pinch-LASL/1973
Tokamak(PLT)-PPPL/1975
Laser Implosion Glass & DT-KMSF & LLL/1974
Tokamak-PPPL/1973
Laser Implosion CD_2 Sphere-Lebedev/1974
10^{-6}
10^{-7}
10^{-8}
10^{-9}
10^{-10}
1
0
-1
Log_{10} T_{ion}, keV
10^{10}
10^{11}
10^{12}
10^{13}
10^{14}
10^{15}
Log_{10} $n\tau$, sec/cm^3

FIGURE 3

NON-TECHNICAL HURDLES TO FUSION POWER — LLL

	Then (1945-50)	Now (1970-75)
• Negative thinking	"There is no conceivable reason why we need this thing, and we should forego attempting to acquire it. Existing means are adequate for all time to come. It is very probably impossible, in any event."	"We don't need this advance, and we do need the money proposed for it for other, much more valuable programs. It may be impossible to do, anyway, or practically impossible for another half century."
• Small-mindedness/ short-sightedness	"We will eventually reach this goal, long before there is any National need for it, and the present rate of advance is perfectly adequate. We need no increase of effort or funding."	"We have finite resources, and we must have a balanced National program in this whole field. The present rate of funding is commensurate with recent progress in this area, its potential, and available resources."
• Trust in incompetent Government planners and advisers	"The work that was done at Los Alamos and elsewhere during the War was such a collossal achievement that it will be at least a quarter century before it can possibly be duplicated by any other nation." (1945)	"There will be an abundance of oil at close to presently prevailing prices through the end of the century." (1970)

FIGURE 4

PARALLEL PATHS TO THERMONUCLEAR POWER? LLL

Situation and Developments	Military (H-Bomb)	Civilian (CTR)
Unappreciated; validity of basic concepts doubted by most	1942-45	1950-58
Abandoned by many key workers	1946	1960-62
Kept barely alive by efforts of faithful few	1946-49	1960-71
Revived through keenly perceived National need; political opposition bested; apparent carte blanche for program	1949	1972-75(?)
Overwhelming resources brought to bear—p-rsonnel, technical areas; exploitation of new and re-worked basic concepts	1949-51	1975-80(?)
Massive overdesign of experiments—nothing left to chance when possible:		
GEORGE—"proof-of-principle", but fuel burn very weak relative to igniting means, as expected	1951	1977-79(?)
MIKE—"engineering breakeven", with design behavior achieved	1952	1980-85(?)

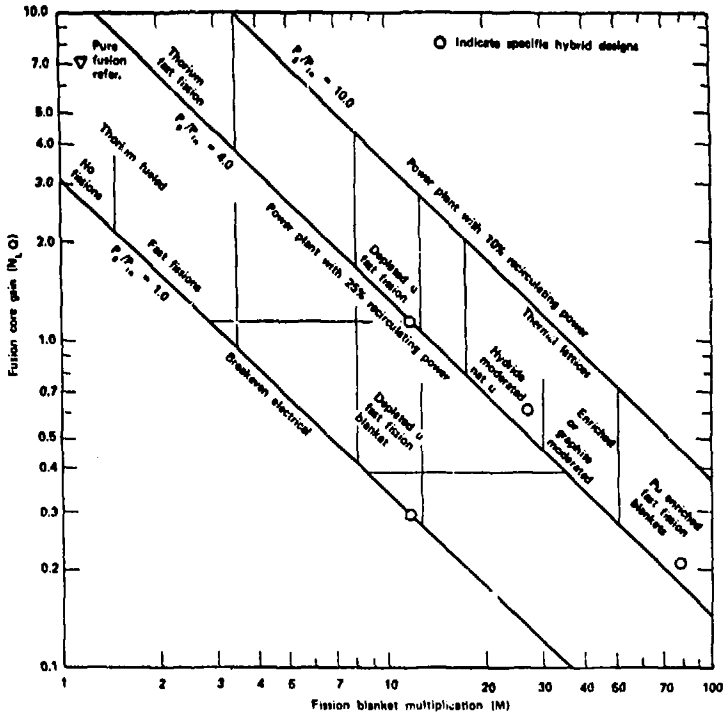
FIGURE 5

PROPOSED TIMETABLE FOR AN ACCELERATED CTR PROGRAM LLL

Date	Milestone
1977-8	• Scientific breakeven—$n\tau \geq 10^{14}$ sec/cm^3, with θ_i > 5 keV
1978-9	• FERF initial operational capability (IOC)— $\geq 10^{14}$ 14 MeV n/cm^2-sec
1980-1	• Fuel-producing fusion-fission hybrid IOC— > 2 fissile atoms generated/ 14 MeV neutron and tritium breeding $\geq$ 1.1
1982-3	• Fusion test reactor IOC— $\geq$ 10 MW thermal fusion power, no electricity generation
1983-4	• Engineering breakeven—Fusion power $\geq$ 10 x electrical power to plasma generator
1984-5	• Fusion power reactor IOC— $\geq$ 10 MW electrical power generation, net • Fuel and power-producing fusion-fission hybrid IOC—send-out electrical power $\geq$ circulated power; tritium breeding $\geq$ 1.1; $\geq 10^3$ tons/year of fissile material generated, net
1986-7	• "Shippingport-level" fusion power plan IOC— $\geq$ 300 MW electrical output, commercial in all respects except cost
1990-1	• Economic breakeven—commercial demonstration fusion power plant IOC with $\geq$ 300 MW electric output

FIGURE 6

LASER FUSION CORE GAIN REQUIREMENTS FOR HYBRID FUSION-FISSION SYSTEMS



$$M = \frac{3.57}{N_L Q}\frac{P_e}{P_{in}} - 0.20, \text{ for } N_T = 0.35$$

FIGURE 7

FEATURES OF AN ACCELERATED CTR PROGRAM — LLL

- Massive education and training program
  - Build up university base
  - Integrated, long term production plan for needed science and engineering talent
  - Foster sources of new concepts
  - NASA model
- Extensive computer utilization
  - Build and validate physics simulation code packages of wide validity
  - Evaluate all ideas exhaustively with relevant simulation tools
  - Output all valid ideas to experimental validation
  - CTR cybernetics—laser fusion example
- Maximally time-compressed experimental programs
  - Program stages run in parallel to predecessor and successor, to maximum extent
  - Multiple sources, program paths, problem solutions
  - Personnel deployment flexibility
  - Realistic, goal-oriented program management
  - Planned, control waste to purchase speed
  - DoD and NASA program management techniques—Polaris, nuclear Navy and Apollo examples
- Highly parallel and redundant programs
  - All viable concepts carried through engineering breakeven level
  - All "critical paths" through engineering breakeven redundantly covered
    - With existing technology wherever possible
    - With advanced technology wherever possible

FIGURE 8

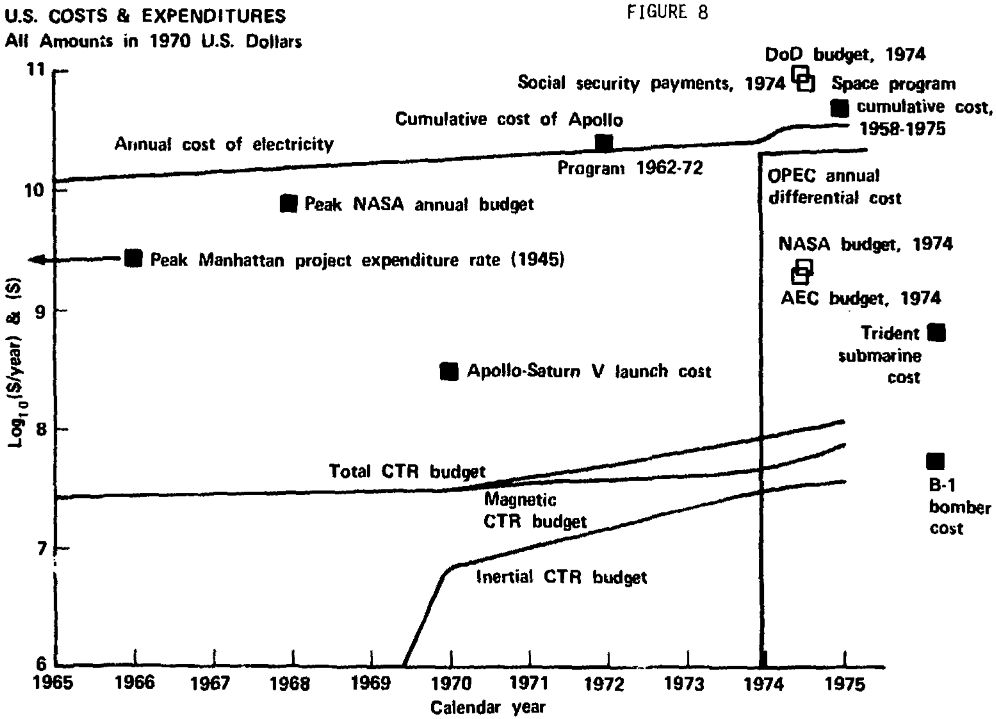

U.S. COSTS & EXPENDITURES
All Amounts in 1970 U.S. Dollars
DoD budget, 1974
Social security payments, 1974
Space program cumulative cost, 1958-1975
Cumulative cost of Apollo Program 1962-72
Annual cost of electricity
OPEC annual differential cost
Peak NASA annual budget
Peak Manhattan project expenditure rate (1945)
NASA budget, 1974
AEC budget, 1974
Trident submarine cost
Apollo-Saturn V launch cost
Total CTR budget
Magnetic CTR budget
Inertial CTR budget
B-1 bomber cost
Log10($/year) & ($)
11
10
9
8
7
6
1965
1966
1967
1968
1969
1970
1971
1972
1973
1974
1975
Calendar year

FIGURE 9a

THE BASIS FOR CONFIDENCE IN THE CTR PROGRAM — LLL

	Magnetic	Inertial
• Experimental results	$n\tau$ products within 2-3 orders of magnitude of Lawson breakeven, at kilovolt ion temperatures	$n\tau$ products within 1-2 orders magnitude of Lawson breakeven, at kilovolt ion temperatures
• Theoretical power	Advanced theoretical and analytical capabilities, currently being integrated with high level computing capability	Very advanced theoretical capabilities, tightly interlaced with very high level computing capability of known relevance and validity
• Theoretical-experimental agreement	Available theory in semi-quantitative agreement with experimental results	Theoretical-computational results in quantitative agreement with all implosion experiments
• Predictive value and results of theory	Theory often of great predictive value; predicts Lawson $n\tau$ values in next generation machines, under construction	Theoretical-computational means sufficient to predict all proposed experiments; predict "scientific breakeven" with experimental systems under construction
• Theoretical trends	Toward more quantitative and detailed prediction of experimental results; will accelerate sharply as integration with exponentiating computing capability proceeds	Towards universality of predictive capability, and in ever greater detail; strongly dependent on ever greater computing capability
• Experimental trends	Very rapid progress in last 2-3 years in toroidal systems; moderate progress in all other areas. Lawson breakeven projected for 1977-78	$n\tau$ results comparable to those of magnetic CTR achieved in first year of experimental work. Lawson breakeven projected for 1977-78

FIGURE 9b

	Magnetic	Other	Inertial
• Richness and variety of approaches and concepts	Toroidal/CW Toroidal/Pulsed Cylindrical Implosive Mirror Pinch Focus	E-Sheath Electrostatic Microwave	Laser Electron Beam Microwave Focus Plasma [Other]

• Breadth and depth of technological base

Materials sciences	—super-alloys, isotopically pure structural materials, materials compatability at high temperatures; superconductors and cryogenic materials and engineering
Diagnostics	—ultra-high speed instrumentation of all types; computer control and monitoring of experiments with multi-hundred channel data lines; IC technology, miniaturization
Quantum electronics & optics	—very advanced optical components, instrumentation and systems; quantum generators, amplifiers, and detectors—broad band, low noise, great dynamic range and sensitivity
Electronics	—digital and analog devices, from chips to huge computer systems; instrumentation of low cost and extreme sophistication

o
o Etc.
o

• Technical talent and leadership

- Extremely popular, highly visible field—more so than any other in applied physics or engineering, at present
- Huge influx of bright, young, highly motivated people underway
- Strength-in-depth in seasoned workers
- Goal-oriented, technically competent, highly motivated, politically adept leadership

FIGURE 10

"What criteria can be used to decide whether a new technology is "ripe" for exploitation on a large scale? What methods are most effective for appraising the state of the art to determine the feasibility and timeliness of a major technological effort?"

Committee on Science & Astronautics, U.S. House of Representatives, to the National Academy of Sciences, 1966

"The correct answer to this question would indeed solve all our problems... There are only three pieces of advice that one can offer in a general sense. One is to give a chance to the capable monomaniac. The Wright Brothers were capable monomaniacs; Admiral Hyman Rickover is a living example. Any moderately able and motivated person can succeed in a "ripe" and recognized field, but it takes a special person to cultivate the uncommon seed."

Edward Teller, in Applied Science and Technological Progress, a report of the National Academy of Sciences to the Committee on Science and Astronautics, U.S. House of Representatives, 1967

"When a collection of Grand Old Men in any technical field tells you that something can be done, they are almost invariably correct. When they tell you that something cannot be done, they are nearly always wrong."

Arthur Clarke